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A REVIEW OF DECISION THEORETIC LITERATURE WITH IMPLICATIONS REGARDING GOVERNMENTAL RESEARCH AND DEVELOPMENT POLICIES

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This project began as a literature search for generalizations about the nature of communications and information within the R&D decision processes which might be relevant to the development of federal policies regarding research and development. We were looking for ideas and formulations, either formal or informal, which appeared to be true (or at least accepted by a "reasonable" set of professionals) and which could serve as a methodology or as a possible support for some general, relevant propositions. Several different academic and professional areas were surveyed. These included economics, management, operations research, decision theory, information science, engineering, political science, and accounting. The major journals and books in each of these fields were surveyed in some detail.

It became clear very early that the proper focus of the project was not upon "communication" and information in general. The more relevant aspects of the general problem dealt with decisions and how communication and information bear upon decision making processes. If communication and information have any effects relevant for policy purposes, surely such effects are upon the actual decisions made. The performance of the R&D sector, which is the primary object of policy

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analysis, will reflect the decisions of those involved in R&D, and these decisions in turn will be determined by the patterns of information and communication and the nature of the decision processes themselves. We could make no interesting statements at all about communication or information without considering in detail the means by which the information might be transformed into a decision. Consequently the focus of the project became decision processes and the related decision theory.

No one will be surprised to learn that we found no panacea to the problem of designing federal R&D policies. Problems abound, but we are willing to draw four types of conclusions: (1) conclusions about the literature; (2) conclusions about the R&D phenomena which can be inferred from this literature; (3) conclusions about potential policies; and (4) conclusions about potential research developments in these areas. In addition, from time to time we offer our judgments and opinions about a variety of propositions which on the basis of current knowledge cannot be absolutely confirmed or rejected.

The paper is divided into four major sections. The first part is a survey of the decision theoretic literature. Four different types of decision processes or models are described in general terms. Problems involved with the applications of the models are outlined. The second section examines the extent to which results found in the literature can be applied directly to the design of a federal R&D policy. The third section draws some inferences from this literature about the nature of the R&D process which seem to be relevant for policy. The final section summarizes the conclusions.

I. THE DECISION THEORETIC LITERATURE

The potentially relevant literature is immense in size and breadth. It appears primarily under the headings of management, engineering and engineering management. Relevant discussions can be found in many disciplines, including accounting, economics, statistics, psychology, political science, sociology and business. It varies in complexity from anecdotal business stories to applications of the more recent developments in stochactic optimization theory.

Of the several ways of possibly partitioning this literature, we have chosen to focus upon the structure of the decision making process. We could have partitioned the literature along the lines of academic disciplines; or according to the place in the overall R&D process the decisions are usually made; or according to the formal tools used to describe the decision process. These classification schemes are not really independent since there has been some tendency for the disciplines to specialize in both the type of tools applied to the problem of understanding R&D decision processes and in the type of processes examined. Thus our first category, optimization models, has been examined most extensively in the engineering management literature. The second type of process, scoring models, has been examined most extensively from a structural point of view in the political economy literature, even though the applications appear in the management literature. The third class, cost-benefit and production function models, has been developed almost exclusively by economists. Screening and committee processes, the last category, have received attention in a tangential way from management science, sociology and political economy.

A. Optimization Models

Optimization models are designed to answer the question: How does one decide upon the "optimum" strategy when confronted with competing research proposals? The form of the question carries two implications which are reflected in the literature. First, the theory is developed from the point of view of an individual decision maker. Secondly, the detailed models usually pertain to the problem of project selection.

Researchers primarily intend for the models to help an individual decision maker. The purpose of the research appears to be to provide models which can serve as decision aids rather than models which detail the behavior of some ongoing process. How can the individual organize his affairs so as to attain some well-specified result? In this respect the literature can be viewed as an adaptation of the statistical decision theory found in [DeGroot, 1970], [Fishburn, 1970] or [White, 1967], to the special case of R&D decisions. The particular decision maker who might use these models is frequently supposed to be a manager working in the environment of a profit, or at least goal, oriented organization.

Many models are available. Cetron, Martino and Roepcke [1967] describe over thirty different models and cite over 150 papers on models in the bibliography. Detailed summaries of selected models can be found in [Cetron, et al., 1969], [Baker and Pound, 1964], [Gear, Lockett and Pearson, 1971], [Souder, 1972], [Beattie and Reader, 1971], [Lucas, 1971].

Optimization models are of the following general form. First a set of outcomes, O, are identified and to each outcome $x \in O$ a number $W(x)$ is attached. The function $W(x)$ is called the "objective function," "values," or "weights," and it is the actual mathematical expression that is maximized by a programming technique. The outcomes frequently have the interpretation of "goals" or "objectives." Some authors demand that this set have a type of internal structure such as independency. The weights, then, are interpreted in some value sense. They are always numbers, and carry all of the quantitative implications of numbers (very little care is exhibited on this point -- contrary to what one finds in economics [Chipman, 1971], [DeGroot, 1970], [Fishburn, 1970]).

Projects are described in terms of a set of characteristics or features, C. Exactly what goes into this class varies from one procedure to another, but it can contain characteristics such as initial cost outlay, qualifications of the investigator (in terms of papers, citations, peer ratings, inventions, patents), probable return to capital, and time lapse prior to results. Once C has been established, it is connected to O by means of some transformation $T: C \rightarrow O$. Summarizing, we have for each project $j \in J$, when J is the set of proposed projects, a set of descriptions C(j) which translate into outcomes T(C(j)) which are then valued W(T(C(j))). The problem is to maximize W(x) for $j \in J$ subject to $x = T(C(j))$. This type of model is certainly familiar to economists. We will outline two simple examples.

Example 1. (the case of selecting only a single project)

Objective index set: $G = \{1, 2, \ldots, .\}$

Objective importance: $0 \leq v_i$, $i \in G$ and $\sum_{i \in G} v_i = 100$.

Project index set: $J = \{1, 2, \ldots\}$

Project j's contribution to objective i: $.1 \leq y_{ij} \leq 1$.

Project j's probability of success: Pr[j].

$$\max_{j \in J} \left(\sum_{i \in G} v_i y_{ij} \right) \Pr(j) \frac{1}{\text{cost}(j)}$$

Example 2.

Consider a research on a product capable of providing several different services. Let the types of services it can perform be indexed by $i = 1, 2, \ldots, N$.

v_i = (\$ cost per unit of service i when using a competitive product) minus (\$ cost per unit service i when using our potential product).

x_i = units of service used.

Value of $j = \Sigma_i \ x_i v_i$.

Aside from complexity, what seems to be the problem? There are clearly practical and analytical problems. These are well recognized [Lockett and Gear, 1972], [Kazanowski, 1968], [Roberts, 1969]. Aside from these, there are two additional problems which detract from the potential usefulness of these models at a national policy level. The first problem, the problem of determining the objective function, arises with the transfer of these techniques to the social level. The second problem arises from the use of probability numbers.

All of the models use some quantitative means of determining success. In the case of a firm this can make a lot of sense. The business organization, as such, has a rather clearly defined purpose. Few would argue that when all the other tests fail, one can theoretically resolve competition between projects in terms of their ultimate contribution to profits. In the case of a social setting, one must first determine the allocative implications of the various R&D projects. The analog of profits in this social setting would be the social preference or social welfare if such quantities could be systematically defined. The problem here is that all attempts to define such things have resulted in impossibility results [Arrow, 1963], [Fishburn, 1973], [Plott, 1972]. These results point to a fundamental theoretical problem (in addition to any practical problem) which stems from the application at the group level of a theory developed for an individual decision maker. The most current research holds not even a glimmer of hope for a solution.

The second problem is also of a fundamental nature. The models require some probability assessment to be attached to success indicators. Typically, these probabilities are obtained from a series of experts. The question that needs to be raised pertains to the adequacy of a number such as probability to accurately carry information about an expert's attitude about the success prospects of a project. Even more serious questions exist about what is involved when one compares such numbers between experts.

For concreteness, let's consider the following procedure. There is a set, G, of goals, e.g., the gadget works in all products; the gadget can be produced with existing equipment; etc. There is a set, E, of experts and a set, J, of proposed projects. From each expert, $i \in E$, we get a matrix $\left[P_{ijk}\right]$ $j \in J$, $k \in G$, indicating the probability, in his opinion, that project $j = 1, 2, \ldots$ will attain the given goal k. These data are transformed, then, into an "evaluation" P_{jk} which indicates for each project, j, the "real probability" that it will achieve goal k.

Notice now that we are discussing projects which have never been attempted before. Each project is unique, so there can be no verified relative frequency of success to which the expert can refer in providing his probability estimate. Consequently, we may be willing to accept as meaningful only the qualitative aspects of his opinion. We might accept his statement that "j is more likely to be successful than j' " as meaningful, while at the same time be unwilling to accept the full implications of his statement that "j is 20% more likely to succeed than j'." The latter statement requires a much more complicated and involved type of probability and belief system.

If we are dubious about the quantitative aspects of subjective probability, what can we say? Again, for the case of three or more experts, we have found ourselves in the middle of impossibility results. To be precise, we state the following reformulation of a well-known impossibility theorem.

Let $(L_1, \ldots, L_n)$ be the rankings by $n \geq 3$ experts, according to their opinion about the relative likelihood of success of $K \geq 3$ projects. Now by using "rankings" we avoid the use of quantitative probabilities as attempted in [Allen, 1968], see also [Jones, 1969]. We seek an aggregate ranking $A(L_1, \ldots, L_n)$ such that:

(1) $A(L_1, \ldots, L_n)$ is defined for all $(L_1, \ldots, L_n)$;

(2) if j is judged more likely than j' by all judges, then j is judged more likely than j' under $A(L_1, \ldots, L_n)$;

(3) the relative likelihood of success of j and j' under $A(L_1, \ldots, L_n)$ depends only upon the judges' opinions about j and j' and not on some other project j'';

(4) there is no overriding "best judge" in that if he thinks j is more likely than j', then j is deemed "more likely" than j' regardless of the opinions of the other judges.

It can be proven that no aggregation procedure has all of these properties.

This means, essentially, that when aggregating across experts, one must be using (perhaps implicitly) the more quantitative properties of expressed opinion. On the other hand, it is safe to say that we do not, at any level, understand the possible quantitative structure (or lack of such) of personal probabilities.

These questions would be academic if the quantitative subjective probabilities were successfully used often enough to justify any general claims of success in the selection of projects, but this is not the case [Gee, 1971]. They would also be academic if an experimental methodology or theoretical structure were on the horizon which would allow good and poor models to be unambiguously differentiated, but this is not the case either. The few tests of the accuracy of subjective probabilities have produced ambiguous results [Souder, 1969; Meadows, 1973], and the tests of selection models that use probability estimates in the "field" have been so few that generalizations cannot be made [Souder, 1973]. From the experimental point of view, Delphi techniques are receiving a lot of attention [Fusfeld and Foster, 1971], [Pill, 1971]. The Delphi technique involves a methodology for achieving some consensus over the relative probabilities without interference of personality factors. If consensus can always be assured, the impossibility result could be eliminated by eliminating condition 1 above. At this stage, however, one could not honestly propose the use of Delphi as part of a national R&D policy.

On the theoretical front, we have measurement theory [Krantz, et al., 1971]. As a theoretical approach to problems it has the capacity to untangle the idea of personal probabilities in a manner useful for R&D project selection purposes. This area is apparently not receiving much attention outside of psychology right now. Consequently, the theoretical works are designed to meet the needs of that discipline rather than R&D.

B. Scoring Models

In a mathematical sense scoring models subsume most of the other models we have discussed. From an abstract point of view, almost everything, many optimization models included, become scoring models. We have no need for such an abstract development here so we will stick to the descriptions as they are found in the literature. Scoring models as applied to R&D decisionmaking are well presented by Moore and Baker [1969a and 1969b].

In essence a scoring model as applied to project selection is much like a balloting system. In this respect it is not surprising that much of what is known about the structure of such models comes from political science and the closely related field of social choice theory [Sen, 1970], [Fishburn, 1973]. The idea is that projects should receive scores or points (or votes) in accord with how they stack up on several different criteria.

From a theoretical point of view it is known that all scoring systems can exhibit very peculiar behavior. From a behavioral point

of view, with one exception, [Pessemier and Baker, 1971],[1] almost nothing is known. Rather than survey the potential types of scoring systems and their related problems, we will examine only two types and demonstrate some of their unusual features.

Many scoring models involve rank order voting at some stage. Projects in a fixed set are ranked according to predetermined criteria. Consider the criteria:

A. Success predictions
B. Test facilities and capabilities
C. Contribution to other ongoing projects
D. Quality of personnel
E. Stability of personnel level
F. Reliability of deadline estimates
G. Consistency with general funding objectives

Each of four projects, w, x, y, z, are then ranked according to these seven criteria listed on the table. Each project is assigned a score, for each category, equal to its rank in that category.

	Criteria								Aggregate Score	
	A	B	C	D	E	F	G		Case 1	Case 2
rank 1	x	w	x	y	w	y	w	w	13	
2	w	z	w	x	z	x	z	x	18	15
3	z	y	z	w	y	w	y	y	19	14
4	y	x	y	z	x	z	x	z	20	13

[1] This paper advocates the use of dollar metric data for the evaluation of projects. In essence, the preferences of judges are measured in terms of what they would pay for various projects. It has been shown elsewhere (Lichtenstein, 1971] that this particular measure of preference can systematically contradict other measures of preference. The full implications of these facts for the dollar metric approaches are yet to be explored.

The top ranked gets a score of 1, and the lowest ranked gets a score of 4. An aggregate score is computed for each project by adding together the scores it received on the individual criteria. The project with the lowest score wins.

The aggregate scores are shown on the right of the figure as Case 1. Clearly project w wins with 13 points, and it is accepted. Now if we had sufficient money remaining for another project, we would be tempted to spend it on project x; it is second in line. Obviously we would hesitate to spend it on z, which had the worst score. Yet, if we recompute the scores with w eliminated, we find that z obtains the best score. The totals are shown as Case 2.

Let's take another example. Suppose seven judges named 1, 2, ..., 7 respectively, are asked to evaluate three projects x, y and z. Each judge is to grade each project according to a five-point scale. If the project is excellent it gets 5 points. If it is high average, it gets 4 points; average gets 3 points; low average gets 2 points, and poor projects get 1 point. The points given to a project by the seven judges are then summed. The project with the highest sum is chosen.

The ratings by the judges are shown on the table below. It reflects the apparent behavioral fact that judges tend to assign their median ranked alternative to the "average" category.

Projects by Assigned Class by Judge

Judge	Excellent	Above Average	Average	Below Average	Poor
1		y	z	x	
2		x	y	z	
3		y	z	x	
4		z	x	y	
5		x	y	z	
6		z	x	y	
7		x	y	z	

The aggregate scores are 22 for x; 21 for y; 20 for z. Clearly x is the "best" and z is the "worst" according to this model. x will be chosen and implemented.

Suppose, prior to the final tabulation, a new proposal w is received and passed along to the judges for their evaluation with the rest. It is not a very good proposal in that each judge rates project z higher. Suppose in fact that each judge ranks the new project just below z but just above whatever alternative was previously just below z. For judges 2, 5 and 7, z was previously low, so the new project is now lowest.

Of course, with w present, some of the judges must rescale some of the alternatives. We suppose they tend to avoid the lowest categories (this has been reported as a behavioral fact [Nordhauser, 1971]. The rescaling is shown in the table below.

Judge	Excellent	Above Average	Average	Below Average	Poor
1	y	z	w	x	
2	x	y	z	w	
3	y	z	w	x	
4	z	w	x	y	
5	x	y	z	w	
6	z	w	x	y	
7	x	y	z	w	

The total scores are 20 for w; 25 for x; 26 for y; and 27 for z. The clear winner is z which was the previous loser, and the loser (aside from w which is really low) is x which was the previous winner. The introduction of w reversed the ranking between x, y and z, even though each judge's rank of x, y and z remained constant.

Now do these examples mean that scoring models should not be used? No. They mean that scoring models are delicate things the full implications of which are not likely to be understood by the user. Until much more is known about such models we would not suggest using them as a basis for legislation or widespread administrative actions.

The examples simply serve as signals. The structure of the scoring procedure can influence decisions in ways the user may not anticipate. From a mathematical point of view one can show that "peculiar" behavior is not at all unlikely. From an actual behavioral point of view the likelihood of behavior such as demonstrated by the examples above has never been investigated.

C. Production Function Studies

What is the R&D program doing for us? To the extent that R&D is a program, separately funded and composed of many separate projects, this is certainly a legitimate question. Funds used for R&D could be used for something else including other R&D programs. Should the R&D division receive more money this next year? Should Federal research dollars go to physics or should they go to economics? Clearly these are important problems and they are the type of problems that production function models and cost benefit models are intended to help solve.

There are two separate problems involved. First there is a means/end problem. What are the relationships between the controls we have available and the pattern of results we see? If we want a particular result do we know how to achieve it with the means available? The second problem involves evaluation of the ends. Would we know what we want even if we knew how to get it? So the first problem relates to "how" we might go about achieving certain ends, while the second problem is one of assigning values to ends.

Program evaluation is difficult. Some people suggest simple rules (e.g., "does the competition surprise us"), but these seem inadequate and the problem remains hard for several reasons. First, because R&D takes a long time, it is very difficult to decide upon a time reference. We can conceptually determine today how good the program was five years ago; but how do we determine today the quality of the program today, when the results will not become known for five more years? In addition, the data are usually bad. There are even theoretical problems which stem from the nature of joint costs. For example, a good R&D program can aid in recruiting by increasing corporate prestige, serve as a training ground for employees, and serve as a general source of "scientific intelligence" for the organization.

This latter feature, in turn, can be a catalyst to a cross-fertilization of knowledge among divisions used in operating procedures and in evaluating the competition. A good R&D group can even help sales by providing a fundamental understanding of the firm's products as well as competitor's products. All of these benefits are potentially there but are almost impossible to measure. It is apparently necessary to lump all R&D expenditures together even though they are likely to have contributed to almost every aspect of the organization.

Almost all approaches in this class involve some type of econometric model. The model $\Delta P_t = a \Delta R_{t-k} + B$ is a popular form, although the variables may differ between applications.

P_t = (sales, profits, productivity, market share, cost savings, earnings, etc.) at time period t.

R_{t-k} = R&D expenditures at time t-k where k is a constant indicating the lag between expenditures and results.

a, B = constants to be estimated by the procedure.

Managers also keep an eye on certain statistics such as revenue from new products/revenue from all products; costs/profits; percentage of products that experienced a quality improvement; and number of patents. Occasionally these statistics are reviewed with an eye toward the claims made in preproject proposals.

Models like this are found most frequently in the management and engineering management literature [Horowitz, 1963], [Taymour, 1972]. They are suggested there for use in the evaluation of R&D programs. The theory behind the equation is clearly that R&D effort in some sense influences or determines sales.

Curiously enough, the inverse of this function is used in the industrial organization literature of economics and it is accompanied by the exact <u>opposite</u> theory. Rather than R&D expenditures producing sales, it is presupposed that sales (or size) produces R&D. The theory

that larger firms undertake more R&D is relevant to policies that might affect the size of firms.

Which theory is right? Which way does causality go? Grether [1974] claims that all attempts to resolve this dilemma are unsuccessful. This claim, if correct, clearly casts doubt upon the use of the management models as a backbone for R&D program evaluation at this time.

The simple regression model does not exhaust the literature. Brockhoff [1970] uses a linear programming approach to estimate parameters indexing the productivity of research input. The reader should also reference Minasian [1969] and Mansfield's [1972] general survey.

This problem of determining the relationship between input and output is characteristic of both cost-benefit studies and cost-effectiveness studies. In addition, these studies attempt to evaluate the program by evaluating its contribution to some (already evaluated) outcome.

The major (implicit) critics of these procedures are those who feel that evaluation should be devoted primarily to inputs. The reference [Edwards and McCarrey, 1973] contains a good survey of studies involving the measurement of an individual researcher's performance. Overall performance as measured by peer rankings, quantity and quality of written output as measured by citation counts and peer ratings, and various measures of the creativity of output are discussed as possible indicators of performance. However, the various studies reviewed tend to disagree on which variables correlate, and all of the studies seem to adopt a measure of performance which has an obscure relationship to the type of performance the employing organization requires. Part of the problem here is the lack of a definition for the latter type of performance. These authors conlude that performance cannot be measured by one criterion -- if indeed it can be measured at all.

Another source of implied criticism is found in the accounting literature. There are differences of opinion within that discipline. For example, [Newman, 1968] argues that accounting data are much too poor to use in an evaluation. Other scholars are more "hard line" on accounting. Budgets should be prepared and management should judge performance relative to the prespecified goals. The following quote is taken from [Sasaki, 1969]:

> A carefully prepared budget is essential in planning the over-all R&D program. It is the only device which summarized detailed man-hour schedules, equipment schedules, purchasing plans, personnel acquisition forecasts, etc., in a way that both technical and nontechnical executives can easily understand in terms of dollars and cents. Without this formal device to focus attention on research plans, the R&D program would tend to drift into those areas of greatest interest to individual scientists, or into pet projects of certain managers. In using concrete program planning it is possible to lower the cost of research by eliminating duplicate research effort, by weeding out undesirable projects, by focusing scientists' attention on research costs and the need for research productivity, and by anticipating otherwise unforeseen difficulties. To gain these advantages, the development of an R&D budget can be divided into four stages:
>
> 1. Setting a target figure
> 2. Initial project budgeting
> 3. Project co-ordination by research executives
> 4. Review by top management

The third source of criticism arises over the assignment of values when the discussions move to the social realm. We have covered this problem above. The theoretical problem of social valuation applies here as well.

D. Screening and Committee Processes

A decision process which involves a sequence of hurdles or hierarchy of decision makers will be called a screening process. In these processes the projects undertaken are not the ones "selected" -- they are the ones which survive. We know of no general survey or

study of screening processes even though they appear to be frequently used in industry. We are also unable to find a means of judging what a systematic study might reveal without getting into the whole area of the behavioral aspects of laboratory management (see survey by J. P. Martino [1973]). Consequently we must drop this important topic and move to narrower considerations.

Frequently the decision procedures involve judgment by committee. The very fact that a committee is involved may exert its own influence. We turn then to some observations on committee processes.

There is reason to suspect that groups make riskier decisions than do individuals [Wallach, 1962]. We are not aware of a field test of this proposition, but it would appear that R&D would be an appropriate setting. If laboratory results carry over to the more complicated arena, we would have reason to suspect that organization of decisions along committee lines might influence, toward greater risk, the nature of accepted projects. We know that committees whose members have divergent interests tend to make less accurate decisions than groups whose members have harmonious interests [Bowers, 1965]. Organizations which put divergent interests on committees in order to foster "organizational harmony" may well do so at the expense of efficiency.

Many are unaware of the potential pitfalls of organization along committee, as opposed to individual, decision lines. The general feeling of managers appears to be that committee procedures involve no more than a combination of a balloting procedure with periods where those members of the organization who hold extreme positions are to persuade or be persuaded by the other members. However, laboratory results indicate that members with the correct view will adopt an incorrect view when subjected to group pressures [Asch, 1951]. Methods which force consensus may well achieve little more than a "median" opinion.

Balloting procedures have an independent influence. For example, the "reversal" phenomenon exhibited in the second example of a scoring model, above, is clearly applicable to the group decision case. Majority rule also has "perverse" attributes. In particular, it can result in inefficient choices. Suppose there are three committee members (a, b, c) and four projects (w, x, y, z). The preferences of a, b, and c are -- where the alternatives are listed in order of preference -- w, x, y, z; y, z, w, x; and z, w, x, y respectively. They eliminate w in favor of z; a majority favors z. A majority favors y over z; and then a majority prefers x over y. Thus, they choose x. However, everyone prefers w over the chosen alternative x.

All balloting processes have "problems," the nature of which are investigated most systematically in the axiomatic social choice literature of economics and political science. There are also some attempted laboratory tests [Birnberg, Pondy and Davis, 1970]. No generalization other than that -- relevant to R&D management -- can be made at this time. We simply call the reader's attention to these phenomena in the hope that he will not proceed on the assumption that certain facets of decision analysis take care of themselves when, in fact, they do not.

II. DIRECT POLICY IMPLICATIONS OF THE DECISION THEORETIC LITERATURE

Much of the research on research in this area is structured around a rather narrow managerial point of view and is of questionable relevance to matters of broad social policy. Very little from a social or economic point of view is offered, with exceptions to this rule being papers published in economics or works commissioned by the government for the purpose of the development of science policy. The preponderance of literature is devoted to the development of techniques to aid managers

in solving certain types of reasonably well-defined problems. As methods they may be helpful at points of policy application, but the conclusion to be drawn at this stage is that the literature has not been devoted to the development of behavioral regularities which could potentially serve as the basis for national R&D policies. Such behavioral regularities that have been isolated were done so because of their relevance to the solution of particular managerial problems. The potential generality of behavioral results has not been of great importance to researchers and consequently remains almost completely unexamined. If a fact will not help make a few bucks it is likely to go unappreciated in the areas which we have reviewed.

There are two fundamental modes by which national R&D efforts can be organized. The following quote catches the essence of the choice:

> In management-centered organizations the problems and tasks facing the concern as a whole are broken down into specialisms. Each individual pursues his task as something distinct from the real tasks of the organization, as if it were the subject of a sub-contract. "Somebody at the top" is responsible for seeing to its relevance. The technical methods, duties, and powers attached to each functional role are precisely defined. Interaction within management tends to be vertical, i.e., between superior and subordinates. Operations and working behaviour are governed by instructions and decisions issued by superiors. This command hierarchy is maintained by the implicit assumption that all knowledge about the situation of the firm and its tasks is, or should be, available only to the head of the firm. Management, often visualized as the complex hierarchy familiar in organisation charts, operates a simple control system, with information flowing up through a succession of filters, and decisions and instructions flowing downwards through a succession of amplifiers.
>
> Entrepreneur-centered systems are adapted to unstable conditions, when problems and requirements for action arise which cannot be broken down and distributed among specialist roles within a closely defined hierarchy. Individuals have to perform their special tasks in the light of their knowledge of the tasks of the firm as a whole. Tasks lose much of their formal definition in terms of methods, duties, and powers, which have to be redefined continually by interaction with others participating in the task.

> Interaction runs laterally as much as vertically. Communication between people of different ranks tends to resemble lateral consultation rather than vertical command. Omniscience can no longer be imputed to the head of the concern. [T. Burns, 1967]

Both modes of organization involve the use of goals. With the former, management-centered goals are clearly specified at the top. With the latter, the goals are left largely unspecified. To the extent that they exist at all, they are implicit in the incentive structure (as opposed to the "duties") of an organization.

Is there an objective means of setting goals at the national level? The answer to this question at the practical level is No. At the theoretical level the answer is No. At the philosophical level the answer is probably No. This conclusion is of major importance. Logic and order have a very great appeal and it is easy to believe that a panacea for resolving questions of judgment exists if only it can be found. This belief tends to foster self-deception. Individuals tend to adopt complicated methodologies when they cannot see the full ramifications.

The reasons for the negative answers above revolve around the lack of an accepted definition of social preference. If an accepted definition could be found then we would need only to formulate management standards and controls which would assure the socially most preferred pattern of R&D activities as a result. Now in the case of a firm, this might not be hard to do. There is certainly a harmony of interests among owners. At the social level, however, such harmony is lacking. R&D results which help one group are likely to harm some other group; consequently, thinking at the national level by analogy to an individual or a firm can get one into trouble.

The following summarizes the thinking of a managerial group about the means of establishing and managing national R&D activities:

> Briefly, the allocation process may be summarized as a process of assigning weighting factors according to a methodical procedure; applying the Program Weighting Factors to the Normalized Program Value Curves for each group of Programs associated with a Field; allocating to the Programs within a Field fund dollars to derive Normalized Field Value Curves; derive Weighted Field Value Curves by applying the Overall Field Worth factors to the Normalized Field Value Curves; allocating the assumed Federal R&D budget, minus such items as agency sustaining funds and agency discretionary funds, to the various Fields, utilizing the Weighted Field Value Curves; and, finally allocating the field funds to the Programs within a Field by the allocation process utilizing Weighted Program Value Curves. [Study Report, UCLA, 1970]

The key idea for us to dwell upon is the concept of "weights." The role of such weights is to induce values on competing R&D activities. That is, they implicitly induce a preference, at the social level, on R&D allocations and thus are of maximum importance. How are such weights to be determined and by whom? It is unlikely that Congress or some high level advisory committee would assign numbers to abstract entities without a rather complete understanding of the implications. Besides, presumably at this stage Congress does not know or have an opinion about patterns of research activity. Most likely at this stage it is seeking advice as to what it should or might want rather than seeking to implement those wishes it has already articulated. The weights, then, must come from somewhere else.

Supposedly, they will reflect the preferences of the society at large. But this answer brings us back to our initial problem. How does one define the "social preference"? A review of the balloting procedures discussed above should serve to alert the reader to how really complicated and deep the problem is. Clearly, the procedures outlined above will not do. What about other procedures? One can prove they all have difficulties. This is the major implication of research specializing on these problems. [Plott, 1972] Concepts which have a social preference as a background condition are extremely limiting and should probably be avoided.

This discussion has some practical implications apart from the methodological. Expressions such as "social preference," "social utility," and "social welfare" should be used with reservation in the actual policy formulation. They are not likely to lead to restrictions on behavior which are not already imposed by the general legal structure. If all that one intends is for decision makers to "consider" someone other than himself, the terms might be useful. As terms intended to convey a rather narrow course of prescribed actions, however, these are likely to fail and such a failure can be costly.

There is, for example, a tendency to use such vague words in attempts to direct firms toward problems related to public needs (e.g., transportation, education, health care). The lack of market creation in areas where public funds determine size, characteristics, and timing of market demand, was identified at [A. D. Little, Inc., 1973] to be a major barrier to innovation. The recommendation was to "Formulate performance criteria (technical, economic, social, institutional) considering both public needs and industry's delivery capabilities, in order to clarify demand and characteristics" (p. 81). Researchers should be made aware of the pitfalls which linger with concepts such as group preference. These notions may be useful at the individual or firm level, but they are not really useful at the social level. The problem with using many of the methods found in the management-oriented literature is that they generally presuppose the existence of a social preference.

Are there tools which might help improve the coordination of governmentally sponsored research efforts? Are there tools developed in this literature which, for a given outlay, would help governmental research directors (a) improve the sequencing of research efforts in order to anticipate research demands, (b) coordinate research efforts in order to take care of complimentarities, (c) reduce the time between decision and use by making researchers aware of "related" projects and researchers? The answer is, in a sense, Yes. Models exist which help organize information and which systematically call attention to a

large number of facts. Generally the models are not considered to be accurate predictive devices, but they are frequently regarded as helpful [Morgenstern, Shephard, Grabowski, in Yovits, et al., 1966].

Input-output techniques can be modified for R&D analysis [Morgenstern, Shephard, Grabowski, in Yovits, et al., 1966], [Dean, 1967]. The idea is to develop a table representing the "interactions" between different fields of science. Another table models the interaction between various fields of science and technology with the remainder of the economy. Such a model, at a conceptual level, can be used to identify potential bottlenecks in research efforts as well as areas of research which could be in demand as a result of a change in the composition of the production output. Models such as this, as well as those mentioned in the next paragraph, can be supplemented with trend analysis and/or Delphi techniques. Both have been used considerably for "futures" analysis; and both provide useful information about likely future events. Neither is widely proclaimed as a reliable means of modeling technological development, however.

Many adaptations of critical path and linear programming methods of optimization exist. These models relate to the efficient attainment of prespecified goals. Morphological analysis and relevance trees form the greater part of such techniques [Jantsch, 1967]. The basic idea is one of specifying the goals of an entire R&D division. Efforts are then made to define feasible ways of obtaining these goals. The techniques are designed to facilitate, primarily, the latter task and provide an algorithm for solving the optimization problem. Consensus in the management literature would not be favorably disposed to these methods. The reader should consult a reference such as [Cetron, Martino and Roepcke, 1967] or [Beattie and Reader, 1971] for a more positively inclined evaluation.

III. INDIRECT IMPLICATIONS OF THE LITERATURE

There is not a method, technique or scientific law in these areas which can simply be applied in some direct fashion as a backbone for a national policy. However, from the discussions and models we can draw some generalizations about the nature of R&D which may be relevant. In particular we are willing to draw some conclusions about the nature of the "externality" relationships in R&D which can be used in the design of market and nonmarket institutions.

Examination of the R&D literature on research and development induced the following classification of R&D projects: (1) exploratory, (2) high risk business development, and (3) support for existing business. This classification scheme, developed by Gee [1971], does not amount to an "iron law" of R&D management. The classes seem to be meaningful to managers and many R&D managers are prepared to make generalizations about the nature of projects and problems within a given category. There are alternative or competing classifications, see [Quinn, 1949; Gordon, 1966; Ackoff; 1966].

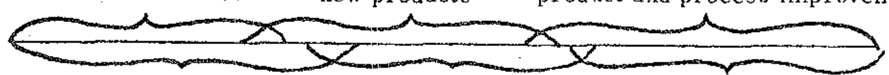
To us, this classification suggests two different dimensions along which one might think about R&D projects. The first pertains to the likelihood of successfully attaining stated research goals. The second dimension has to do with how narrowly research purposes are focused. Exploratory projects are those which are risky and which have "broad" implications in terms of the number of products and processes potentially effected. Projects in the second category are ones which are risky but have a narrow focus, while the third category contains low-risk, narrowly-focused projects. We hesitate to use expressions such as "degree of risk" and "narrowness of focus" since an underlying, quantitative system of measurement seems to be implied. No accepted system exists. Nevertheless, we suspect that "similarity" rankings of

a given set of projects along these dimensions would show a great deal of stability across experienced R&D managers. That is, we suspect that these intuitive ideas, risk and focus, capture the structure on which analogies and judgments about the similarity of projects are based.

R&D output can also be classified according to the user. There are three categories of users of interest to us: (1) other basic researchers, (2) technology-oriented researchers who work on new products, and (3) technology-oriented researchers who are focused upon product improvement and cost reduction techniques. Of course, these classes are bound to overlap somewhat. But the literature under consideration supports the contention that the overlap is minimal [Ackoff, in Yovits, et al., 1966]. More importantly, it seems to be the case that projects classified in category (1) in the first classification would be used by group (1) in the second category. In other words, the results of exploratory research are used primarily by other researchers. A diagram might be helpful:

Intended Users and Use of Research Output

(researchers) further research | (technologists) new products | (technologists) product and process improvement



high risk and broad focus | high risk and narrow focus | low risk and narrow focus

Nature of Research Activity

This dichotomy, when taken together with some empirical relationships, suggests a structure of externality or "public goods" relationships. The empirical relationships have to do with the means by which the transfer of research output from one group to another takes place. This aspect of R&D -- the transfer -- differs according to whom the users are.

In particular, scientists tend to use written means of transfer, while technologists tend to communicate orally [Allen, 1966a and 1966b]. It appears that as one moves from the left of the spectrum, where research is exploratory, to the right, the means of "effective" communication changes from written papers to oral communication. In a sense, this is easy to understand. The application of technology requires a familiarity with many special features of a problem. The use of a person in the transfer of technical knowledge allows immediate, selective, creative responses to statements. People placed face to face have all of their senses at work. In addition, there are factors of trust and belief which operate in the transfer and acceptance of information and which personal relationships tend to resolve. The special features of problems can be better meshed with the special features of knowledge by people working together.

This communication feature of R&D means that the structure of outputs designed for basic researcher use -- the high risk, broadly focused research efforts -- tend to have the characteristics of a type (not the classical type) of public good [Plott and Meyer, 1974].

Due to the intrinsic characteristics of the "fundamental" type of research, even if publication could be prevented and other controllable means of communicating knowledge substituted for it, an efficient market for "basic" results could not be established. The output of "basic" research is often not seen to be of value -- even by the researcher who produced it. Such output must be "cast out" to the scientific community for judgment, before value or even validity can be established. Often, only if some result of scientific research is first seen by a researcher will that researcher realize the result would make a valuable contribution to his own work. Thus, scientific knowledge constitutes a commodity that must be "consumed" before it can be purchased at a price accurately reflecting its worth. However, once "known," there is clearly no reason to buy knowledge. Ergo, any market for

ideas at the broadly focused research level would be inefficient -- expensive to maintain and enforce.[1]

So, we find that research which has broad implications ("basic" research?) tends to have the attribute of a public good. In addition, it tends to be of value. Shotwell [1971] indicates, for the group studied, that the scientific literature was the major source of new product ideas. The fact that the information was in written form tends to support the idea it was "basic." Utterback [1971] has noted that ideas tend to come from outside while solutions tend to come from inside. Presumably, "solutions" are on the "nonbasic" end of knowledge. Bartocha, Narin and Stone [1970] concluded in the case of several new products, that about 70 percent of the key events that led to development were non-mission oriented and occurred about twenty years prior to use, many of which presumably consisted of "basic" discoveries. Three empirical studies that also indicate the value of "basic" research are reviewed by Price and Bass [1969]. It is true, however, that this general problem of the value and use of basic research has not received attention sufficient to assure a thorough understanding of the problem. For example, this general line of argument seems inconsistent with the results of "Project Hindsight," where it was concluded that most of the results necessary for the completion of several military innovations were accomplished because of the mission alone [Isenson, 1968]. Results along these lines are also reported in [Baker, Siegman and Rubenstein, 1967].

As we move in the direction of projects with a greater focus, we begin to leave the public good aspects of the research results. If transfer of technology is primarily accomplished by person-to-person contact, exclusion is clearly possible. The value can in principle be

[1] Example: Would Einstein have put an ad in the paper offering to purchase Riemannian geometry from someone so he could develop his theory of relativity? Would he have known how useful Riemannian geometry would be to him if he hadn't already known it?

captured by the one who creates it. The possible case for social policy, here, is of a different nature.

The scientific nature of focused research can be recognized and it can thus dictate the type of inputs the research project needs from a broader scientific background. Technologists can identify those individuals who are likely to have relevant information. Likewise, researchers with a broad background are capable of identifying special cases of their own expertise. Once the potential benefits of cooperation have been recognized, regular market relationships such as consulting contracts can serve as the organizing institutions. The indivisibility of an individual may have some implications. It might br hard to purchase just that aspect of a researcher which the customer desires. Institutions such as variable term consulting contracts, part-time employment, and people loans have evolved, in part, to solve this problem.

This aspect of information flow does contain some hints which need to be pursued. Professional meetings, journals and associations are all activities of a "public" nature which are likely to be very influential on the overall innovative activity within the country. Physical ties between research groups could possibly be encouraged with a consequential increased level of innovative activity. The strategy of supporting education and research in the universities may have been an important contribution to the U. S. technological successes. Such contributions could have resulted from the research findings and successes directly; but, more importantly, educational support advanced the creation of a very large number of researchers in the economic system who could serve as the means of transmitting information in the economy in general between the nonfocused, researcher-oriented researchers and the focused product-process oriented researchers.

IV. IMPRESSIONS AND OPINIONS

A. What Can This Area Do For Social R&D Policies?

If an overall model exists which could serve as a basis for national R&D policies, it is not in the literature we surveyed. It is not difficult to find claims to the contrary, but in our opinion such claims are simply wrong and are based on an incomplete understanding of the many deep and complicated issues.

Many techniques can be found in this literature which could be useful to any R&D decision maker when organizing facts prior to decision or in understanding the consequences of previous decisions. We found no evidence of a single or set of "best" techniques which could be recommended across the board. Rather, the best technique appears to depend heavily upon the technical capabilities of the decision makers and the particulars of the decision situation.

B. What Should Be the Government's Role in R&D?

First, we think this literature indirectly lends support to the view that governmental financial assistance should be concentrated on the "basic" end of the management, physical and social sciences. This is where market institutions seem least able to organize resources appropriately.

Secondly, a case can be made for student support. Apparently a high level of training among the population at large is a very important catalyst for technological advancement. It makes market institutions function "smoother" at the development end of the research spectrum. We do not know whether or not this benefit of federally supported university research has been fully appreciated.

Thirdly, management activities at the national level should be limited to coordination and related activities. Supervision and evaluation should come from other sources. Institutions and incentives should be structured so that the "customer," the one who is the user of the research (in the case of basic research the "customers" are simply other researchers), should be an active participant in choosing the directions of research support. The government should influence the choice between competing projects and areas indirectly by deciding who the recipients are to be and including them in the allocative process. This means that in the case of basic research the judgment between competing projects should be made by other basic researchers. The peer review process, for example, developed by NSF, emerges very favorably from this line of analysis. Formulation of detailed purposes should be "decentralized." Influence on the direction of research should be expressed by those who legislators feel should be the recipients of research results.

Finally, there seems to be reason to believe that market institutions can adequately deal with research at the development end of R&D. Educational support and support for means of scientific interaction at a "face-to-face" level (meetings, etc.) might be justifiable on efficiency grounds. Governmental involvement with technology development at the very "applied" level cannot be supported from this line of analysis unless the technology itself is to be used by the government.

C. What Kind of Research on the R&D Process is Needed?

In keeping with the spirit of this section, we will bluster forth with an opinion on the type of research on research which is needed. It is our opinion that in the areas we have reviewed there is a great need for basic theoretical and experimental work. Of the hundreds of papers on the nature of research examined by us, only a small percentage would qualify as dealing with the matter at the basic or broadly focused level -- many of these are cited here. The preponderance of written works provide anecdotes and ad hoc theories. There exists a

plethora of opinions but the instances of integrated theories, replicable results and precisely formulated models are very sparse indeed.

Existing theoretical works are full of potential but the body of theory lacks the refinement, breadth, consistency and general acceptance which accompanies large-scale scientific attention. In part this may reflect a general lack of financial support for basic research in the social scientific areas over the years as compared to the support experienced by the physical sciences. Regardless of the reason, however, it is a lack of work at a foundations level which prevents one from confidently extrapolating and systematically applying the implications of the theories which are found in these areas to questions of national policy.

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